

COMMENTARY ON THE HYMAN-HONORTON JOINT COMMUNIQUÉ

BY REX G. STANFORD

The Honorton-Hyman joint communiqué is a very significant and salutary development. It gives some substance to the hope that reasonable parapsychologists and reasonable external critics do not live in separate universes, that there are realities we share that can provide the common ground for communication and, ultimately, if we are fortunate and science and reason prevail, even the daring dream of a degree of consensus. There is a sense in which science represents a set of basic rules by which individuals can reach consensus about empirical matters. One way of summarizing the outcome of the Hyman-Honorton exchange, reflected in the joint communiqué, is to say that heretofore there has been no specified agreement about the basic rules, whereas there is now at least some agreement. As a consequence, the prospect of using those agreed-upon rules to work toward consensus has become an immediate prospect. This alone has set the discourse on a plane that is genuinely constructive, whatever may be the nature of any ultimate consensus or the time required to arrive at one.

There might also be, between the lines of the joint communiqué, a foundational agreement that was not made explicit, but which I would like to explore here. It concerns why either of the parties—the parapsychologist or the critic—would spend time and effort examining, discussing, and trying to reach agreement on these basic rules. Does it not suggest that both parties agree that it is really important that such claims be assessed on grounds of the scientific canons of evidence, not by rhetoric per se? If so, and if that perspective finds common agreement among both “parapsychologists” and “skeptics,” it should lead to less heated discussion and more research, provided that all the parties, like Hyman and Honorton, can agree to certain basic rules of inquiry.

I do, however, wish to make clear my own feeling that all parties should bear in mind the delimited nature of the specific exchange between Hyman and Honorton. They have been concerned with replicability of performance on ESP tasks in ganzfeld and with how

performance on such tasks should be interpreted. Whatever additional research and evaluation comes out of their joint communiqué, it should be clear that it is relevant to the question of replicability of ESP-task performance during ganzfeld. It seems to me that these parties have *not* agreed that they have laid down ground rules for a test case of the existence of anomalous communication (so-called "ESP") generally. Their exchange and joint communiqué focus on a highly specific empirical claim. This is as it should be. The question is whether a communications anomaly occurs during ganzfeld stimulation. Since we are not presently in the circumstance of testing a theory that specifies the conditions under which such anomalous communication should and should not occur, the outcomes of this research program do not address the truth or falsity of a specific conceptualization of anomalous communication. Therefore, whatever the empirical resolution of the ganzfeld-ESP controversy, the conclusions should not be generalized beyond that methodological paradigm.

I am made quite uneasy by the readiness of Hyman and Honorton to make a *cause célèbre* out of the issue of anomalous communication during ganzfeld by proposing a large-scale systematic replication series under the auspices of the National Science Foundation (NSF). Such a suggestion is incredibly premature and could prove both wasteful and hurtful. My own considered judgment is that success with the ganzfeld-ESP paradigm depends very heavily on a number of variables that are implicit in much of the work done, but not explicit in the written reports. Indeed, those variables might be difficult or impossible to identify or verbalize at present, and they may represent a complex combination of factors. Such factors might include subject population differences, objective laboratory-specific circumstances, and differences in the treatment of subjects, especially in aspects of social interaction (Stanford, 1985). The reasons for success or failure with the ganzfeld-ESP paradigm simply have not been pinpointed, meta-analysis notwithstanding, and there is need for much systematic research here. I am hopeful that researchers can identify critical factors through such work and, possibly, thereby enhance the replicability rate. Nevertheless, the difficult, plodding work required for such purposes has barely begun, and, meanwhile, there is the evidence that investigators differ in their rates of success with the ganzfeld-ESP paradigm (in terms of effect size), as indicated in the Rosenthal paper in the current issue of this journal. There is a really troublesome and sticky problem here that must be confronted.

This is not to suggest that one must have 100% replicability before proposing an NSF (or comparable) study. It is to suggest that we need to have some clear indications about the critical factor(s) in ganzfeld-ESP success such that pretty much any investigator can have a modicum of success with the paradigm. Instead, we have indications of differential success across investigators, very minimal systematic work that might help to pinpoint the critical factors in ganzfeld-ESP success, and almost no systematic work aimed at delineating a conceptual understanding of ESP-task performance in this and other internal-states paradigms (Stanford, *in press*). Such circumstances are not propitious ones for the NSF-type, large-scale work proposed by Hyman and Honorton. It seems, frankly, absurd to ask NSF or any scientific body to undertake a large-scale investigation under such tenuous circumstances.

Given such circumstances it is all the more foolhardy for parapsychologists to advocate such an investigation, for it is virtually certain, in my opinion, that any failure of that effort would be interpreted as a final and telling test of the reality of anomalous communication. And that would be fine if the circumstances of such a test were actually at hand, but they would not and could not be. What one would have to test would be a particular construct to explain such anomalous effects, but work is not at that stage, and this is not what is being proposed. Instead, what the proposed test must look like to any outsider (and, possibly, to many parapsychologists) is a test of the alleged best case for "ESP" that parapsychologists have made so far. Seen as such, negative outcomes would almost certainly be construed as a justified grounds for dismissing this field entirely and the cutting-off of future funding. If parapsychologists are willing to gamble their entire field on the basis of the very spotty pattern of success with ganzfeld when the critical factors for success in that paradigm are unknown, then they are more brash than reasonable.

Instead of the bravado that tempts investigators into such premature challenges, one would hope for the readiness to do the necessary work to delineate realistically boundary conditions for anomalous communication. One hopes, too, for the specificity of conceptualization that will allow the testing of particular explanations of anomalous communication. Part of the problems parapsychology has had have derived from trying to prove the reality of an anomalous effect without adequate investment of funding, time, and effort in delineating the boundary conditions for such effects—and they must surely exist if the effects are real—and in using such

research to develop positive, testable conceptualizations that can either be refuted or supported by subsequent work.

It is true that proof of some "effect" is conceivable in the absence of detailed knowledge of boundary conditions and, certainly, in the absence of conceptual understanding of it. Alas, that is a dream most applicable to work with highly replicable phenomena. Another way to state this is that the test conditions for the effect are known. Work with the ganzfeld gives the illusion that we know something about the test conditions for anomalous communication. In fact, those conditions are not known, much less the mediators for the effect. The effect appears and disappears quite unpredictably. This is hardly the basis on which parapsychologists should hope to convince other scientists that they are concerned with some aspect of reality, and if through ganzfeld research as it presently stands they are not in the position to reap the benefits of such convincing, surely they would not be ready to assume the costs of failure at such an effort.

Researchers who believe that the ganzfeld paradigm as it presently stands is a good basis for convincing the broader scientific community to take parapsychological claims more seriously seem to me to be placing their faith in what is, in a sense, a magical formula for success rather than a scientifically derived one. More politely and perhaps more accurately stated, there may well be more art to ganzfeld-ESP success than there is science. There are, of course, investigators who feel differently about this. Time will tell who is correct.

All of this is not to deny the considerable importance of the work that has been done with ganzfeld and of the efforts that Hyman and Honorton and others have put into reviewing the current status of that work and in arriving at some shared conclusions and excellent recommendations regarding reporting and methodological standards. That is all to the good. There is now more knowledge about where this problem area really stands. What I am really trying to say here is that the suggestion about NSF large-scale work in this area is not the next best step. I sincerely hope and trust that the day will come when researchers in the anomalous communication area will be ready to provide something resembling a scientifically derived formula for ESP-task success to be tried by any scientist who so wishes. I just do not believe that such a juncture has arrived in the case of the ganzfeld-ESP paradigm as it currently stands. I also think it is dangerous to imagine that it has.

What is needed in lieu of a *cause célèbre*, an immediate test case of the ganzfeld-ESP paradigm paraded through the august halls of the NSF, is some more years of hard, systematic work of the types discussed above that make use of the high standards of methodology and reporting advocated by Hyman, Honorton, and other investigators in this area. If, given the apparent promise of paradigms like ganzfeld and hypnosis for ESP-task success, such systematic work bears fruit, then investigators could much more reasonably shout their challenge at the portals of all scientists and academics, NSF included. If it did not bear fruit, then I, for one, would not need NSF and the rest of the scientific community to tell me to stop wasting my time and attend to more useful matters. But I am presently optimistic about these areas and see the failures of parapsychology to develop positive knowledge about its subject-matter more as a function of insufficient systematic work than of a nonexistent or intrinsically intractable problem (Stanford, in press).

The systematic work that I am advocating would, by its very nature, examine both the psychological and parapsychological consequences of the total setting known as ganzfeld. It is difficult to conceive of understanding the parapsychological consequences of ganzfeld without understanding its psychological consequences. Parapsychologists shall thus have to be contributing to psychological as well as to parapsychological knowledge. The historical disjunction of parapsychological from psychological investigation has proven counterproductive.

With regard to implementing such systematic investigation, it is to be hoped that the legitimate concern of Hyman and Honorton with reporting and methodological standards will not be construed as a call for methodological rigidity. Some standardization is useful and even necessary for progress, but too much tends to ossify the research and easily becomes methodological imperialism that impedes progress. Creativity, innovation, and high methodological and reporting standards will all be needed for the successful pursuit of the process-oriented work that might both enhance the rate of significant ESP studies and show that what is now justifiably called "anomalous" is, in some degree at least, "nomothetic."

REFERENCES

- STANFORD, R. G. (1985). Toward the enhancement of inter-laboratory and inter-experimenter replicability in psi research. In B. Shapin & L. Coly (Eds.), *The repeatability problem in parapsychology*. New York; Parapsychology Foundation, Inc.

STANFORD, R. G. (in press). Ganzfeld and hypnotic-induction procedures in ESP research: Toward understanding their success. In S. Krippner (Ed.), *Advances in parapsychological research* (vol. 5). Jefferson, NC: McFarland.

*Psychology Laboratory
SB-15 Marillac Hall
St. John's University
Jamaica, NY 11439*